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Volume 3

Issue 5

September 1976

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by

Richard Steiner

*The Loss of Final Consonants in
Babylonian Jewish Aramaic (BJA)*

by

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*This paper is part of the
Proceedings
of the
Second North-American Conference on Semitic Linguistics
Santa Barbara, California
March 25-26, 1974*

ON THE ORIGIN OF THE $\dot{H}\acute{E}\partial\acute{E}R \sim \dot{H}\check{A}\partial\check{A}R$ ALTERNATION IN HEBREW¹

by

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The $\dot{H}\acute{E}\partial\acute{E}R \sim \dot{H}\check{A}\partial\check{A}R$ alternation is limited to nouns with final resonant, because resonants are particularly prone to acquisition of syllabicity, which, in turn, often leads to epenthesis. The construct forms of such nouns underwent epenthesis earlier than their absolute co-allomorphs — early enough, in fact, to be affected by the well-known Hebrew stress-shift — because they lost their case-endings earlier. The original epenthetic vowel was lowered to *a* by Philippi's law. The retention of *e* in *hävél* is due to overlapping of Philippi's law and stress-shift.

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¹I am greatly indebted to Professors Zeev Ben Hayyim, Joshua Blau, Daniel Boyarin, Henry Hoenigswald, and Saul Levin for their comments on an earlier version of this paper. I would particularly like to thank Professor Levin for spending a great deal of time answering questions which I sent him concerning the accentuation of Hebrew *qotol* forms in the Septuagint. His answers forced me to revise my thinking about these forms, and, in the end, to omit my discussion of them entirely.

1. THE *ḤĒḌER* ~ *ḤĀḌĀR* SUBCLASS OF SEGOLATE NOUNS

Scholars have long been intrigued by a small group of exceptions to one of the fundamental rules of Hebrew morphology, viz., the rule which states that the construct-state allomorph of segolate nouns is identical to the absolute-state allomorph. Both the rule and the exceptions were discussed already in the 11th century by Yonah ibn Janāḥ (1886:205-6, my translation):

"Know that that which is of the form CĒCeC, with six points [·: ·:] or with five [·: ··], usually does not change when put in construct with a non-pronominal substantive, for example, *ḥēreṣ miṣrayim*, *dēreṣ yam suf*, *sēfer hat-torā(h)* ... — most of this class follows in the same path. But some of them do change when put in the construct, for example, *ḥēḏer* in *u-va-ḥāḏār miškōvāxō*; *šēyer* in *šōyār ʾālōfexō* although it is unchanged in *wō-xōl peṭer šēyer bḥemo(h)*; *zēraʿ* in *ki-zrāʿ gaḏ huʾ(?)* although it is unchanged in *kō-zēraʿ gaḏ lōvōn*; *nēṭaʿ* (as in *wō-ʿōšō(h) qōṣir kō-mō nēṭaʿ*) in *nōṭaʿ šaʿāsuʿōw*; [*ḥēṣer*] (as in [*bḥ-ḥēṣer u-vō-xōfōn*]) in *wē-ʿewilīm ba-ḥāsār lev yamuṯu*; *yēreḡ* (as in *wō-loʾ(?) noḥar kōl yēreḡ bō-ʿeš*) in *wi-(y)raq dešeʾ(?)* ... But it is also possible that *wi-(y)raq dešeʾ(?)* is the construct of *ṭov ʾaruḥaṯ yorōq* ... although in that case there would not be any evidence for us in it, since it would not be a member of the *ḥēreṣ* class. And I wonder at Abu Zakariyya [Ḥayyuj] when he says that none of this class change except *ḥēvel* in *ḥāvel ḥāvelīm* ..."²

If we eliminate the one uncertain example here (*yēreḡ*), we are left with a group of nouns all of which end in a liquid or ʿ:

<i>ḥēḏer</i>	~	<i>ḥāḏār</i>	'room'
<i>šēyer</i>	~	<i>šōyār</i>	'offspring'
<i>zēraʿ</i>	~	<i>zōrāʿ</i>	'seed'
<i>nēṭaʿ</i>	~	<i>nōṭaʿ</i>	'plant(ation)'
<i>ḥēṣer</i>	~	<i>ḥāsār</i>	'want, poverty'
<i>ḥēvel</i>	~	<i>ḥāvel</i>	'vanity'

Three of the new examples discovered by Ewald (1835:250, 1855:472) fit in perfectly here,

<i>sāḥar</i>	~	<i>sōḥār</i>	'trade, profits'
<i>šēvaʿ</i>	~	<i>šōvāʿ</i>	'seven'
<i>tēšaʿ</i>	~	<i>tōšāʿ</i>	'nine'

²Wa-ʿlam ʾanna-hu mā kāna ʿalā miṭālī peʿel bi-sittati nuqatin ʾay [l.c. Ibn Tibbon ʾaw] bi-xamsatin fa-ʾinna ʾaktara-hā lā yataḡayyaru ʿinda ʾiḏāfati-hā ʾilā l-ʾasmāʾi z-zāhirati miṭla ʾereṣ miṣrayim dēreṣ yam suf sefer hat-torā(h) ... ʿalā ḥāḏā yaṭṭaridu ʾaktaru l-bāb. Wa-qad yataḡayyaru baʿdu-hu ʿinda l-ʾiḏāfati ka-taḡayyuri ḥēḏer fī qawli-hi u-va-ḥāḏār miškōvāxō wa-ka-taḡayyuri šēyer fī šōyār ʾālōfexō wa-ʾin kāna ḡayra mutaḡayyirin fī qawli-hi wō-xōl peṭer šēyer bḥemo(h) wa-ka-taḡayyuri zēraʿ fī qawli-hi ki-zrāʿ gaḏ huʾ(?) wa-ʾin kāna ḡayra mutaḡayyirin fī qawli-hi kō-zēraʿ gaḏ lōvōn wa-ka-taḡayyuri nēṭaʿ ʾaʿnī wō-ʿōšō(h) qōṣir kō-mō nōṭaʿ fī qawli-hi nēṭaʿ šaʿāsuʿōw wa-ka-taḡayyuri bḥ-ḥēṣer u-vō-xōfōn fī qawli-hi wē-ʿewilīm ba-ḥāsār lev yamuṯu. Wa-ka-taḡayyuri wāloʾ(?) noḥar kōl yereḡ bō-ʿeš fī qawli-hi wi-(y)raq dešeʾ(?) ... Wa-rubba-mā kāna wi-(y)raq dešeʾ(?) muḏāfa ṭov ʾaruḥaṯ yorōq ... ʾillā ʾanna-hu laysa yakūnu ḥīna-ʾidīn lanā fī-hi šahā-datan ʾiḏ laysa min bābi ʾereṣ. Wa-ʾinnī la-ʾaʿjabu min ʾabī Zakariyyā fī qawli-hi ʾanna-hu lam yataḡayyar min ḥāḏā l-bābi ʿinda l-ʾiḏāfati ḡayra ḥāvel ḥāvelīm.

but Ewald's fourth new example

qāḥaθ ~ *qāḥāθ* 'taking, to take'

does not. It should be grouped instead with medial pharyngeal forms like:

bā^caā ~ *bā^cāā* 'behind'
**mō^caṭ³* ~ *mō^cōṭ* 'fewness'

An additional example was discovered not long ago by Berggrün (1950:7) in the Kaufmann manuscript of the Mishnah — he could have cited Codex Parma A (= De Rossi 138), as well — and Elijah Levita's *Sefer Ha-Tiṣbi* (s.v.):

sē^cen ~ *sē^cān* 'superior priest'

It should be noted that, although neither of these singular forms occurs in the Bible, the plural form does, and it is therefore likely that the alternation attested in the Mishnah is much older than that source. This example ends in a nasal rather than a liquid, but since the liquids and nasals form a natural class (commonly called resonants), it fits in well with the other examples.

Three other construct forms deserve mention here, although it is not certain that they are actually derived from segolates: *gāḥōl* (Exodus 15:16), *gāḥōah* (I Samuel 16:7), and *qāḥōš* (Psalms 46:5, 65:5). The idea that *gāḥōl* and *qāḥōš* are in some way equivalent to *gāḥēl* and *qāḥēš* (the Hexapla actually READS *κοδσ* in Psalms 46:5) goes back at least as far as Samuel ben Meir (Exodus 15:16), but the suggestion that *gāḥōl*, *gāḥōah*, and *qāḥōš* have something in common with *ḥāḥār*, *ḥāḥēl*, etc. seems to be original with S.D. Luzzatto ([1860] 1970:7-8, [1871] 1965:290; cf. also Lambert 1931:108, Kogut 1969:23-4, Blau 1971:318). There is, however, another school of thought which maintains that *gāḥōl*, *gāḥōah*, and *qāḥōš* are the construct forms of the adjectives *gāḥōl*, *gāḥōah*,⁴ and *qāḥōš* (Brockelmann 1908:II,48, Rabin 1967:7, Koehler-Baumgartner 1967, s.v. *qāḥōah*). Brockelmann (op. cit.) adduces these forms as evidence that adjectives can serve as abstract nouns in Hebrew, a proposition which becomes much more attractive when limited to adjectives in the construct state (cf. also *mar* in I Samuel 15:32, Psalms 38:15, *yāḥaθ* in Deuteronomy 21:11, and perhaps *yāḥe(h)*⁵ in I Samuel 16:12, 17:42). This syntactic explanation obviously implies that *gāḥōl*, *gāḥōah*, and *qāḥōš* have nothing to do with *ḥāḥār*, *ḥāḥēl*, etc., and we shall therefore take the prudent (and convenient) course of omitting these forms from the discussion which follows.

One last alternation which may belong here is the alternation of the infinitive construct, *qāṭōl*, with its co-allomorph (before suffixes), *qāṭl-*, but since the original shape of the

³This form is reconstructed on the analogy of forms like *kōveḏ* 'heaviness', *ḥōzeq* 'strongness', etc. The reconstruction depends on the assumption that *mā^cōṭ* is an abstract noun rather than an infinitive construct, a distinction which is perhaps dubious from a historical point of view but seems to have synchronic validity.

⁴One problem for this theory might be the fact that we expect *gāḥāh* as the construct of *gāḥōah* (on the analogy of alternations like *mizbēah* (abs.) ~ *mizbah* (const.)), and this form actually occurs several times in the Bible. On the other hand, the retention of *o* in *gāḥōah* would seem to be just as much of a problem for those who take it as the construct of *gāḥah*.

⁵The appropriateness of this example hinges on the meaning of *‘im* in the two verses cited. It is generally taken to mean 'and' in these verses, but it might, like English 'with', have the meaning 'possessing (a quality)'. This meaning would fit nicely in I Samuel 25:25 and Psalms 89:14 as well.

latter is uncertain due to fluctuation in the Massoretic pointing (between forms like $\delta\alpha\chi\beta\alpha\chi\alpha$ and $\epsilon\alpha\beta\iota$, which point to an original *CuCC, and forms like $\delta\alpha\chi\nu\sigma$ and $\kappa\alpha\theta\nu\sigma$, which point to an original *CuCuC), we cannot be sure. Moreover, if the vocalization of $q\alpha\tau\alpha\lambda$ owes anything to epenthesis, it owes at least as much to contamination of the infinitive construct with the infinitive absolute (Joüon [1923] 1965:109-10) and the imperfect (cf. Bauer-Leander [1922] 1965:316-7). The original form of the alternation and its conditioning may accordingly be beyond recovery.

A form which does not belong here, contrary to what I once thought, is $\sigma\phi\alpha\rho\theta\epsilon\lambda\lambda\epsilon\iota\mu$, the rendering of $\delta\alpha\lambda\epsilon\iota\mu$ "Book of Psalms" in Origen's list of the books of the Bible, reproduced by Eusebius in the *Ecclesiastical History* (Schwartz 1908:574). The form $\sigma\phi\alpha\rho$ is open to several interpretations. It could be a scribal error for * $\sigma\alpha\phi\rho$ which crept in before the time of Eusebius ($\sigma\phi\alpha\rho$ is definitely the form which Eusebius had; cf. the critical apparatus, loc. cit.), but * $\sigma\alpha\phi\rho$ would be anomalous in its own right as a rendering of Hebrew * $\delta\alpha\lambda\epsilon$ (one would expect * $\sigma\epsilon\phi\rho$ ⁶).

A second possibility would be to take $\sigma\phi\alpha\rho$ as a rendering of the Aramaic construct form⁷ $\delta\alpha\lambda$. It is true that $\theta\epsilon\lambda\lambda\epsilon\iota\mu$ has the HEBREW plural suffix and that all of the other names⁸ on Origen's list are Hebrew (e.g. $\alpha\mu\epsilon\sigma\phi\epsilon\kappa\omicron\delta\epsilon\iota\mu$ ⁹, $\delta\alpha\beta\rho\eta\iota\alpha\mu\epsilon\iota\nu$ ¹⁰), but, as Kutscher has shown (1959:15-6, 46-7), none of this is incompatible with an Aramaizing vocalization. Indeed, Origen's vocalization of the Biblical text itself is not free of Aramaisms (loc. cit.), so there is no reason to exclude the possibility of an Aramaism in his vocalization of the Biblical book-titles. On the other hand, it should be noted that, out of the many segolates in the extant fragments of Origen's *Hexapla* (Mercati 1958), there is not one on the pattern of $\sigma\phi\alpha\rho$ (Brønno 1943:136). Even $g\beta\lambda$ in Psalms 18:26, vocalized $g\beta\lambda\alpha$ by the Massoretes, is vocalized $\gamma\alpha\beta\rho$ by Origen (loc. cit.). In any case, it is obvious that both of the above interpretations obviate the need to deal with $\sigma\phi\alpha\rho$ within the framework of this article.

The third, and most likely, hypothesis is that $\sigma\phi\alpha\rho$ is to be connected with the Samaritan Hebrew form $\alpha\delta\alpha\lambda$ 'book' (the initial vowel is prothetic and probably late). This form is not a segolate since it occurs (Ben Hayyim 1961) in the absolute state (e.g. Deuteronomy 31:24, 26) as well as in the construct (e.g. Deuteronomy 24:1, 3). It is, rather, a noun on the

⁶Cf. forms like $\zeta\epsilon\chi\rho$ (= Massoretic $\zeta\alpha\chi\alpha$ -) and $\rho\epsilon\sigma\theta$ (= Massoretic $\rho\alpha\chi\alpha$ -) in the second column of Origen's *Hexapla* (Mercati 1958). Other forms with ϵ correspond to Massoretic α -stems: $\nu\epsilon\phi\sigma\iota$ (= Massoretic $\nu\alpha\phi\alpha\iota$), $\delta\epsilon\rho\chi\omega$ (= Massoretic $\delta\alpha\kappa\omega$).

⁷The construct still existed at this period (Kaddari 1969:104) even though d -periphrasis had become, in Kaddari's words, "the regular way of nominal subordination" (ibid., 102).

⁸It should be noted, however, that many of these titles "consist of either (1) the first word or words of the book ... or (2) the name of the hero or supposed author..." (Swete [1914] 1968:214), and that such titles cannot be adduced as evidence concerning $\sigma\phi\alpha\rho\theta\epsilon\lambda\lambda\epsilon\iota\mu$ which is "a description of the contents" (loc. cit.). Consequently, only titles which fall into this latter category are adduced here.

⁹This is close to the Mishnaic title of the Book of Numbers, but the vocalization differs. Codex Kaufmann has $\hbar\upsilon\mu\mu\alpha\delta\ \hbar\alpha\rho\text{-}\rho\acute{\iota}\kappa\kappa\upsilon\delta\acute{\iota}\mu$ in *Soṭah* VII,7 and in *Menaḥot* IV,3 and $\hbar\upsilon\mu\alpha\delta\ \hbar\alpha\rho\text{-}\rho\acute{\alpha}\kappa\upsilon\delta\acute{\iota}\mu$ (the *ketiv* is $\hbar\upsilon\pi\upsilon\kappa\upsilon\delta\acute{\iota}\mu$) in *Yoma* VII,1. Codex Parma A (De Rossi 138) has $\hbar\omicron\mu\epsilon\delta\ \hbar\alpha\rho\text{-}\rho\acute{\iota}\kappa\kappa\upsilon\delta\acute{\iota}\mu$ in the *Yoma* passage.

¹⁰Hebrew $\delta\alpha\upsilon\kappa\epsilon$ (= Tiberian $\delta\acute{\iota}\nu\upsilon\epsilon$) $\gamma\alpha\mu\acute{\iota}\nu$ "Chronicles" (lit. 'things of days'). Note that this title cannot be Aramaic because the Aramaic plural of $\gamma\omicron\mu$ 'day' is $\gamma\omicron\mu\acute{\iota}\nu$ with an \acute{o} , and because the Aramaic word for 'things of' is $\mu\acute{\iota}\nu\epsilon$. The plural suffix $-\acute{\iota}\nu$ is, of course, almost as common in Mishnaic Hebrew as it is in Aramaic.

qāṭol pattern, virtually identical to the Late Biblical (II Chronicles 2:16)¹¹ and Mishnaic (*Nazir* VII.3, *Kelim* I.1, *Zavim* V.10) word *ṣāḥar* meaning 'counting'. It follows that, even according to this interpretation, *σφαρ* has little in common with the Hebrew segolate construct forms which are the subject of this article.¹²

2. FOUR QUESTIONS ABOUT THE ORIGIN OF THE *ḤĒDER* ~ *ḤĂḌĀR* ALTERNATION

We are left, then, with a highly coherent set of segolate construct forms, all of which end in a resonant or ' (and it is not unreasonable to theorize, as does Levin (1966:4), that the latter was also a resonant, i.e. an *a*-vowel with pharyngeal constriction but no audible friction, in ancient Hebrew, at least in the environment of C #) and are stressed on their epenthetic vowel. It is obvious that any attempt to explain how these forms originated will have to account for these facts by answering the following questions:

- 1) Why did the stress shift, in these forms, to the epenthetic vowel?
- 2) Why didn't this change take place in the absolute state of *ḥăḏār*, *ḥăvêl*, etc. as well as in the construct?
- 3) Why was the change favored by a final resonant?

One final question is needed to round off this list:

- 4) Why does the epenthetic *e* of the absolute alternate with other vowels in the construct, viz.
 - a) *e* in *ḥăvêl*
 - b) *a* in *ḥăḏār*, *ṣḥār*, etc.

3. PREVIOUS ATTEMPTS TO EXPLAIN THE *ḤĒDER* ~ *ḤĂḌĀR* ALTERNATION

3.1. Sievers' Theory

Previous attempts to explain the *ḥēder* ~ *ḥăḏār* alternation have tended to focus on only two or, at most, three of these questions, question 3 being the one most consistently ignored. Sievers, for example, gives the following explanation (1901b:279) for the alternation:

"Beim Status constr. ist ja auch der Accentwechsel ganz verständlich. Je stärker der Status constr. enttont wird, um so mehr verliert er seine eigene (d.h. historisch berechnigte) Tonsilbe und ordnet sich lediglich dem allgemeinen rhythmischen Gefüge unter, in dem er auftritt . . ."

In other words, the weakened accent of a noun in the construct state was more easily influenced by external rhythmic factors, e.g., the accent of the following noun, requirements

¹¹I owe this reference to Professor Ben Hayyim. In fact, it was he who first called my attention to the form *ṣāḥar* and to the possibility of linking it with *σφαρ*.

¹²The form *σφερ* (τελειμ/τελιμ) which occurs in Epiphanius' list of the Biblical books (Audet [1950]1974:55) and in the genealogically related list published by Audet (ibid., 53) is, of course, even further removed from the forms treated in this article. It is also much more difficult to interpret.

of meter (in poetry), etc. This theory would lead one to expect that proper nouns, which occur only rarely (if ever) in the construct state, would be immune to shift of stress from etymological vowel to epenthetic vowel, and the fact that this is not the case (see §4.2 below) casts a heavy cloud of suspicion over the entire theory. Furthermore, it is apparent that Sievers' theory does not address itself to questions 3 and 4 at all, a defect pointed out, in part, already by Bauer and Leander ([1922]1965:574):

"Die Umfärbung des Ultimavokals bleibt hierbei ... unerklärt."

3.2. Bauer-Leander's Theory

Bauer and Leander's own explanation (loc. cit.) is designed to provide a partial remedy for the above-mentioned defect:

"Wahrscheinlich sind sie einfach nach Analogie der 4. Klasse entstanden:
 $deḇārūm : deḇār = geḇārūm : x; \quad x = geḇār.$ "

The same type of analogy would presumably explain the Umfärbung of $ḥāḏār$, $ḥāsār$, $šōḃār$, $sōḥār$, and $sōḃār$, so even if we do not agree with Bauer and Leander (ibid., 573) that $gāvār$ (Psalms 18:26) is a construct form (see fn.31 below), we are still obliged to consider whether their explanation is capable of accounting for the other forms. Consider, then, the analogy which supposedly produced the singular construct form $ḥāḏār$. Should not the same analogy have produced a plural construct form $*ḥēḏārē$ (< $*ḥīḏārē$ on the analogy of $dīvrē$),¹³ a singular absolute form $*ḥōḏār$ (on the analogy of $dāvār$), and a suffixed singular form $*ḥāḏārō$ (on the analogy of $dāvārō$), especially since the plural construct and the singular absolute are semantically closer to the plural absolute (which stands to the left of the equals sign in the proportion) than is the singular construct.¹⁴

This explanation, then, leaves us in a worse position *vis à vis* question 2 than we were to start with: not only does it fail to answer the question, it actually strengthens it. In addition, it fails to account for the special role played by final resonants in the shift (question 3), or the $\epsilon \sim e$ alternation (question 4a).

3.3. Malone's Answers to Questions 1 and 4a

A third attempt to solve the riddle of the $ḥēḏār \sim ḥāḏār$ alternation was recently made by Malone (1971). Malone answers question 1 (ibid., 53-4) by assuming that epenthesis in the construct forms under discussion (unlike epenthesis in the corresponding absolute forms) took place early enough to feed the general stress-shift which Semitists have long posited for ancient Hebrew (Bergsträsser 1918:114-5, Bauer and Leander [1922]1965:177ff, Cantineau 1931:97, Harris 1939:50, Goetze 1939, Blau 1972:81). Descriptions of this stress-shift vary from scholar to scholar, but all agree that nouns with two etymologically short stem-vowels (e.g. $kavēḏ$, $dāvār$, $enōv$) were originally stressed on the first of these vowels, and that the position of the accent in Massoretic Hebrew (on the second stem-vowel) is a product of the stress-shift (Bergsträsser 1918:114, Bauer and Leander [1922]1965:178, Cantineau 1931:97,

¹³It must be admitted, however, that it is possible to claim that Tiberian $dīvrē$ comes from $*davrē$; see above, fn. 10.

¹⁴One might also ask why analogies of this sort did not change nouns of the form $CeCōC$ (plural $CōCōC-ūm$) to $CōCōC$.

Goetze 1939:442, Blau 1972:81).¹⁵ A segolate noun which underwent epenthesis before this stress-shift would clearly have been part of this larger class of nouns, and would, therefore, have participated in the stress-shift along with the other members of this class.¹⁶ The same conclusion was reached independently by the author of these lines.¹⁷

Malone's answer to question 4a is also new. He assumes (op. cit., 46) that the original epenthetic vowel was *e*, and that this *e* was later lowered to *ε* in unstressed position. Malone does not motivate this solution, apparently expecting his readers to realize that the latter assumption is necessary in any case to account for such alternations as: *šēm* ~ *šem-*, *tošēm* ~ *ʔal-tošēm*, *haggēd* ~ *haggεd-nō*, *yēšēv* ~ *yεšev-nō*.

Malone's answers to questions 1 and 4a are very appealing for the simple reason that they require very few assumptions beyond those required to account for other Hebrew phenomena. In dealing with questions 2 and 4b, on the other hand, Malone (op. cit., 58) offers no solutions beyond those set forth by Sievers (question 2) and Bauer and Leander (question 4b), which, as we have seen, are far from economical. The answers which we are about to give to these questions — and to question 3 — are, the author believes, more in harmony with the multiply motivated solutions to questions 1 and 4a which Malone put forth.

¹⁵Most of these scholars believe that the stress-assignment rule of Classical Arabic (the so-called *Dreisilbengesetz*) originally held for Hebrew as well. This rule assigns the stress in this word-class to the first stem-vowel. Goetze agrees with this result for his own reasons. His position (1939:442-3) is that the first stem-vowel would have been syncopated if it had not been stressed — and the contrast between the treatment of short *i* in *zōrōa* < **ōirā*, *ḥāmōr* < **ḥimār*, and *ṭōḥōl* < **ṭihāl*, on the one hand, and the treatment of that same vowel in *ʿenōv* < **inab* and *šēlō* < **dila*, on the other (ibid., 444) would certainly seem to bear him out. The fact that the Akkadian reflexes of this class are also stressed on the first syllable is further proof, according to Goetze (ibid., 444-5). This latter argument is developed and refined in Steiner 1975 (cf. esp. p. 8).

¹⁶That early epenthesis (in the vicinity of a final resonant!) shows up as a shift of form-class in Tigre as well is clear from Gragg's description (1974:3):

"Te does not tolerate final clusters either, but uses various processes to get rid of them. The most frequent is epenthesis ($\phi > \partial/C\#$: *kalb* > *kaləb*) ...; but shift in form class (*bāql* > *bāqāl*, so frequently for CāCR, where R is a resonant) and deletion of homorganic consonants (*qārṇ* > *qār*) are also attested."

The synchronic repercussion of early SYNCOPÉ is also a shift of form-class, cf. *kəvēd* ~ *kēvēd*, *kəθēf* ~ *kēθēf*, *yərēx* ~ *yērēx*, *gəḏēr* ~ *gēḏēr*, *ʿərēl* ~ *ʿērēl*, *šēlō* ~ *šēla*, *šēʕar* ~ *šāʕar*, *ʿəḏōn* ~ *ʿēḏēn*, *šəxōr* ~ *šēxēr*, *ʔərōx* ~ *ʔērēx*, *ʔəḏēd* ~ *ʔēḏēd* (Steiner 1975:3-4).

¹⁷As Malone points out (1971:60fn58), this same rule-ordering (or "change-ordering") device was employed by Bauer and Leander ([1922]1965:213) to explain the shift of the accent in other classes of nouns (cf. also Goetze 1939:447). It is not entirely clear why these scholars did not extend this solution to the class of construct forms under discussion. It is possible that they were put off by the fact that it leaves the *Umfärbung* of the epenthetic vowel (in forms like *ḥādār*, etc.) unexplained. But then we may legitimately ask why they didn't use the analogy theory IN CONJUNCTION WITH the rule-ordering theory as does Malone. By doing so they would have strengthened their analogy theory, since the probability of any given analogy taking place would appear to rise in direct proportion to the similarity between the analogandum and the analogans. This consideration clearly outweighs the "economy" of allowing one theory (viz. the analogy theory) to account for both *Umfärbung* and stress-shift.

4. NEW ANSWERS TO QUESTIONS 2, 3, AND 4b

4.1. Question 2 (The Stress Shift)

It has often been noted that alternations like $zəqēn \sim zəqān$, internally reconstructed to obey Philippi's law (which is, in turn, based on an internally reconstructed version of $zəqēn \sim zəqānti$, and states that short, stressed, checked, non-low, front vowels become low) prove that final vowels (case endings) were apocopated earlier in the construct state than in the absolute (Bergsträsser 1918:115,149, Bauer and Leander [1922]1965:523, Harris 1939:41, Blau 1972:69,223).

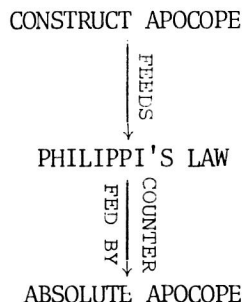


Figure 1

Since the loss of case endings is a necessary pre-condition for epenthesis, it seems reasonable to assume that the construct state was the leader in the area of epenthesis as well:

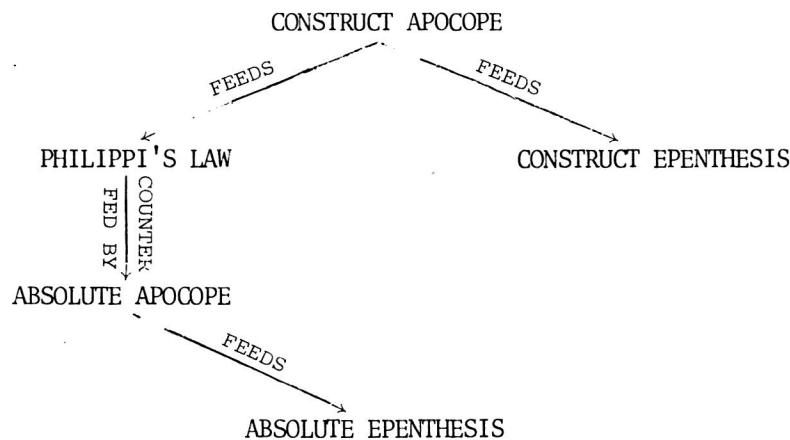


Figure 2

If so, we may answer question 2 simply by assuming that stress-shift occurred after nouns in the construct state underwent epenthesis but before nouns in the absolute state did so:

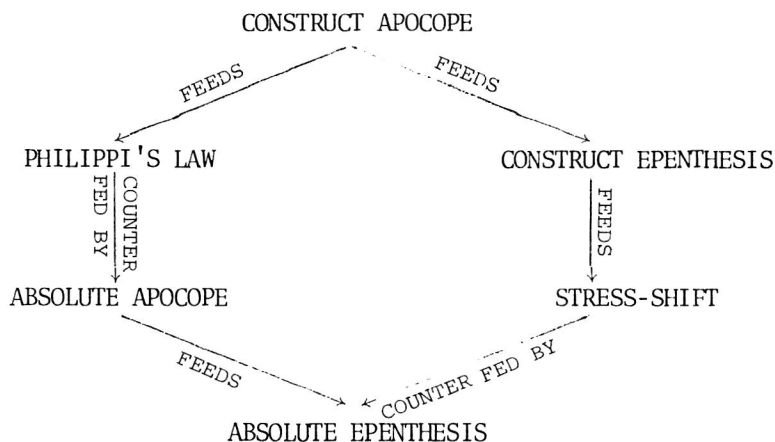


Figure 3

The claim is, then, that the stress did not shift to the epenthetic vowel in forms like *ḥēḏer*, *ḥēvel*, etc. because these forms did not get their epenthetic vowels until after the extinction of stress-shift.

It should not surprise us to learn that epenthesis proceeded in stages. The two stages we have identified coincide more or less with the stages discovered by Bauer and Leander (op. cit., 213). The existence of a third stage has been tentatively proposed (Harris 1941:145), based on the contrast between the behavior of the *bgd kpt* stops following an epenthetic vowel in a noun and the behavior of those consonants following an epenthetic vowel in a verb (cf. especially the minimal pair *lōqāḥaθ* 'to take'¹⁸ : *lōqāḥat*¹⁹ 'you (f.) took'). This contrast would seem to prove that epenthesis took place later in the verb,²⁰ too late to be

¹⁸In Hebrew, infinitives pattern with the nouns because they are derived from verbal nouns.

¹⁹The unspirantized *t* in this form is a regular feature of the 2fs perfect suffix, cf. *šōxāḥat* 'you forgot', *šōlāḥat* 'you sent', *pōṭāḥat* 'you opened', *yōšā'at* 'you knew', *yōyā'at* 'you toiled', *šōmā'at* 'you heard', *higgā'at* 'you arrived', *hišbā'at* 'you satiated', *pōšā'at* 'you transgressed'.

²⁰Just as the early date of construct epenthesis is a function of the early date of construct apocope, so too the late date of epenthesis in *wayyīḥad* (note, in addition to the unspirantized *d*, the unlengthened, or at least unlowered, *i* of this form and others like it in contrast with the *e* of *yehām*) and *lōqāḥat*, *šōxāḥat*, etc. may be a function of the late date of apocope in final-*w, y* verbs and in the second person feminine singular of the perfect. Evidence for the late date of apocope in the latter comes from the unspirantized (and simplex?) *t* of *nōṭat(t?)* 'you gave', in which apocope took place too late for the final *t* to be affected by spirantization (contrast *mattō* 'gift'), either because spirantization was already extinct or because geminate simplification, a prerequisite for spirantization, was already extinct. This theory is not necessarily in conflict with Blau's theory that apocope took place EARLIER in the verb than in the absolute-state noun (1972: 65), because that theory was put forward to account for forms like *šōmār* and *yīxbāḏ* in which we find *a* instead of *o* in syllables which were originally open. There is no good reason for assuming that apocope in *wayyīḥad* and *lōqāḥat* was as early as apocope in *šōmar*.

affected by spirantization.²¹ If so, we obtain a tri-partite division:

CONSTRUCT EPENTHESIS	<i>qāḥāθ</i>
-----STRESS-SHIFT	
ABSOLUTE EPENTHESIS	<i>(lə)qāḥāθ</i>
-----SPIRANTIZATION	
VERB EPENTHESIS	<i>ləqāḥat</i>

Figure 4

It is sobering to realize that, were it not for the fact that two of the sound changes which took place while epenthesis was in progress (viz. stress-shift and spirantization) happened to be fed by it, the Massoretic reflex of **qaḥt* would have been identical in the construct, in the absolute, and in the verb, and we might never have suspected that epenthesis cannot be assigned to only one position in the sequence of Hebrew sound changes. The possibility that there are other such changes of which we are not aware is disturbing.

4.2. Question 3 (Change Favored by a Final Resonant)

The relationship between sonority and epenthesis has long been recognized by linguists. Sievers (1901a:294-5) and especially Jespersen (1913:191ff) have interesting things to say

and *yixbāḥ* — in fact there is evidence suggesting the opposite. It is quite likely, for instance, that in Jeremiah's dialect neither *wayyihad* nor *ləqāḥat* had undergone apocope, cf. forms like *wattizni-šōm* 'and she (!) committed fornication there' (Jeremiah 3:6) and *ʾal-tēmhī* (paroxytone!) 'do not erase (masculine!)' (18:23), for the former, and *ketivs* like *lmdty* (2:33), *qrʾty* (3:4), *šmʿty* (4:19), *hlkty* (31:20), *hrbyty* (46:11) and probably also forms like *šovarti* (2:20) and *nittaqti* (loc. cit.), for the latter. Forms like *sōmār* and *yixbāḥ*, on the other hand, show no sign of having preserved their final vowels in Jeremiah's dialect or any other for that matter. Thus, Blau's theory about the date of apocope in verbs should be restricted to the verb forms from which his evidence is drawn, evidence which, incidentally, can be explained on the basis of accent rather than syllable structure (cf. Brockelmann 1903:9fn1 and Nyberg 1952:§15a,b).

²¹This solution presupposes that spirantization was no longer productive at the time of the Massoretes, but as Blau has pointed out (private communication):

"In Aramaic, at any rate, spirantization was a living feature even in Saadiah's time (v. his commentary to Sefer Yeširah). It is to be assumed that the same was the case in Hebrew, so decisively influenced by Aramaic."

This is a serious objection which I am unable to dispose of in a convincing manner. It is true that it is a long way from the Aramaic vernacular of women in 10th-century Baghdad to the Biblical Hebrew reading tradition of the Massoretes in 8th-century Tiberias, but the gap is narrowed considerably by the existence of segolated but unspirantized verbs in Biblical Aramaic (*hištāḥāhat* 'you have been found', Daniel 5:27) and Babylonian Hebrew (*yāḥad* 'let it rejoice?; let it be united?', Job 3:6 in Yeivin 1973a:54,130). I have decided to retain Harris' explanation because I fail to see any viable alternative to it: the various analogies that might be proposed to account for the unspirantized *t* in *ləqāḥat*, *šōlāḥat*, *poḥāḥat*, etc. are not capable of accounting for the unspirantized *d* in *wayyihad*.

on this subject, but, for our purposes, the most useful discussion is that of Bloomfield ([1933]1965:384):

"When a relatively sonorous phoneme is non-syllabic, it often acquires syllabic function;²² this change is known by the Sanskrit name of *samprasāraṇa*. Thus, in sub-standard English, *elm* [elm] has changed to ['el̩m]. This is often followed by another change, known as *anaptyxis*, the rise of a vowel beside the sonant, which becomes non-syllabic. Primitive Indo-European *[agros] 'field' gives pre-Latin *[agr]; in this the [r] must have become syllabic, and then an anaptyctic vowel must have arisen, for in the historical Latin form *ager* ['ager] the *e* represents a fully formed vowel. Similarly, Primitive Germanic forms like *['akraz] 'field', *['foglaz] 'bird', *['tajknan] 'sign', *['majθmaz] 'precious object' lost their unstressed vowels in all the old Germanic dialects. The Gothic forms [akrs, fugls, tajkn, majθms] may have been monosyllabic or may have had syllabic sonants; anaptyxis has taken place in the Old English forms ['e:ker, 'fugol, 'ta:ken, 'ma:ðom], though even here spellings like *ƿugl* are not uncommon."

That the same relationship between epenthesis and sonority may have existed in Arabic (in the pausal forms of nouns on the pattern CVCCun) was pointed out already by Brockelmann (1908:I, 209) and Schaade (1911:58), and Harris (1936:34) was able to demonstrate its existence in Punic as well:

"Beginning with Punic there are traces of anaptyxis in doubly closed syllables, similar to the occurrence of anaptyctic vowels to simplify the pronunciation of the Hebrew segolates. As would be expected, the words in which this occurs are those in which the last consonant is more sonorous than the preceding, thus making a group which is normally not a single syllable at all and which is very difficult to pronounce. For קבר *qabr 'grave,' there is the Punic variant קבער, with an anaptyctic vowel. In Neo-Punic, *nidr 'vow' is often written נידער; *sīkr 'memory', סכער; and *asr 'ten', עסער."

- Evidence that the relationship may have also held in Hebrew was adduced by Speiser ([1926] 1967) in 1926. (Blake's unsuccessful attempt (1911:219) to prove that Tiberian Hebrew had syllabic resonants in word-final position need not detain us). Speiser observed (op.cit., 390-2) that in Hebrew nouns and verbs which end in a consonant cluster, i.e., forms which should have undergone epenthesis but didn't, the first consonant of the cluster is generally more sonorous than the second (although it is clear that other factors besides sonority are at work in these forms²³). The forms in question are: *nerd* 'nard', *ʿard* 'Ard (pr.n.)', *yerd*

²²Cf. Bell 1970 for a detailed analysis of this change, based on data from a large group of languages.

²³It is no accident that the epenthesis-resistant cluster in four of the forms is *rd* and that it is a voiceless sibilant (generally *ʃ*) plus a voiceless stop (generally *t*) in five or six of the others. Since *r* and *d* are homorganic, epenthesis between them would produce a sequence close to the *C_xVC_x* sequence which many languages tend to eliminate (probably because of the delicate coordination of opposing muscles required to move an articulating organ back and forth quickly, cf. "tongue-twisters" like "Peter Piper picked a peck of pickled peppers") by metathesis (cf. the "2-2 Contraction" rule of Arabic (Hetzron 1974:6-7)), syncope (Bell 1970:12), or haplology. (Counter examples like *mēreð*, *zēreð* (pr.n.) and *sēreð* (pr.n.) result from the momentum that Wang (1969:22) has dubbed the "snowball effect" and do not affect the validity of the argument.) A similar point is made by Sievers (1901a:295):

'have dominion', *yārd* 'give dominion', *qošt* 'truth', *košt*²⁴ 'putchuck', *yešt* 'turn aside', *yešt* 'drink', *wayyašq* 'gave to drink', *wayyevk* 'wept', *yaft* 'make wide', *wayyift*²⁵ 'was enticed', and the entire group *qoštalt* (but cf. *wayyib*²⁶ 'took captive'). From this evidence and some rather too carefully selected parallels in Arabic, Ethiopic, and Akkadian, Speiser deduced (*ibid.*, 392-3) that:

"When two consonants were left in the Semitic languages at the end of a word, there arose the need to develop a secondary vowel in the final syllable if the last consonant was more sonorous than the preceding one. For practical purposes it may be said that this was the case between a stop or sibilant and a following liquid or nasal. Thus arose forms like Arab. *mahāl*, Hebrew **ʾāben* > *ʾēben*, Akk. *šalam*. That there was no phonetic need for the development of a segolate vowel if that order of consonants was reversed is proved by the fact that in modern Arabic there is no anaptyxis under such conditions, hence we get here forms like *qalb*, *milh*. The same is true of the short imperfect of the Hebrew verbs of the type *tertiae* *wy*, hence *yiben* occurs in that language alongside of *yārd* and *yešt*²⁷ And finally, in some languages the vowel was extended to all nouns capable of segolization, notably in Hebrew where **ʾēbed* is now found by the side of *ʾōzen*, *geber*."

"Svarabhakti tritt um so leichter ein, je grössere Schwierigkeiten sich einer raschen Umsetzung der Artikulationsstellung darbieten, d.h. je grösser die Artikulationsdifferenz der Nachbarlaute ist. Zwischen nahezu homorganen Lauten tritt sie daher ausserst selten auf, so etwa zwischen *n* + *d*, *n* + *t*."

As for the failure of *qošt*, *košt*, *yešt*, *yešt*, *wayyaša*, and *wayyib* (if pronounced [wayyišp]) to undergo epenthesis, it is of a piece with the irregular syncopation of vowels in the environments *šš_t* and *š_tt* in languages where syncope is normally restricted to (short) vowels in two-sided open syllables, e.g. P-S **ʾiššatu*- > **ʾištu*-, **ššatu*- > **šištu*- (Steiner 1975:2,5-6,8), Hebrew **šittayim* > *štayim*, Syriac *šittā* > *štā*, and perhaps also Akkadian *ina šattaqad* > Aramaic **štōqaḏ* (Speiser [1926]1967:377fn16). Both of these phenomena, like the metathesis which produced Biblical Aramaic **štūw* 'they drank' (< **šštūw*), reflect a preference for uninterrupted sequences of voiceless sibilant (esp. *š*) followed by voiceless stop (esp. *t*), but the reasons for this preference are not entirely clear.

²⁴A Mishnaic term attested in *ʿUqṣin* III.5. For the vocalization, cf. Mishna Codex Parma B.

²⁵That *yaft* and *wayyift* already had fricative *f* (instead of earlier *p*) by the time verb epenthesis took place follows from the relative chronology proposed by Harris (cf. above, p. 9) on the basis of forms like *yihad*.

²⁶This is probably an exception, but it is also possible that final *b* was devoiced in this word.

²⁷Speiser appears to be less than candid when he contrasts *yārd* and *yešt* with forms like *wattēmer*, *yāven*, *yāfen* (*ibid.*, 391,393), leaving his readers to infer that the law of sonority is a synchronic fact within the realm of final-*y* apocopated imperfects in Hebrew. Actually, there are numerous counter-examples to the law of sonority even in this limited sub-class (e.g. *tireš*, *yārev*, *wayyimeš*, *tēref*, *wayyāref*, *wattēmes*, *tērev* and perhaps also *wayyib*, cf. fn. 26). It is true that all or most of the counter-examples exhibit "over-epenthesis" with respect to the law of sonority rather than "under-epenthesis," but this is a much weaker statement than the one which is implicit in Speiser's discussion.

Speiser's hypothesis is reasonable, even though it goes far beyond the evidence adduced by him. Moreover, it provides a simple answer to question 3, for if segolates ending in a resonant were the first to undergo epenthesis, then it is possible to assume that they were the only ones (aside from segolates with a medial laryngal like *baḏ* and *qaḥt*) which underwent epenthesis early enough to be affected by stress-shift. Accordingly we must modify the diagrams on pp. 9 and 10 as follows:

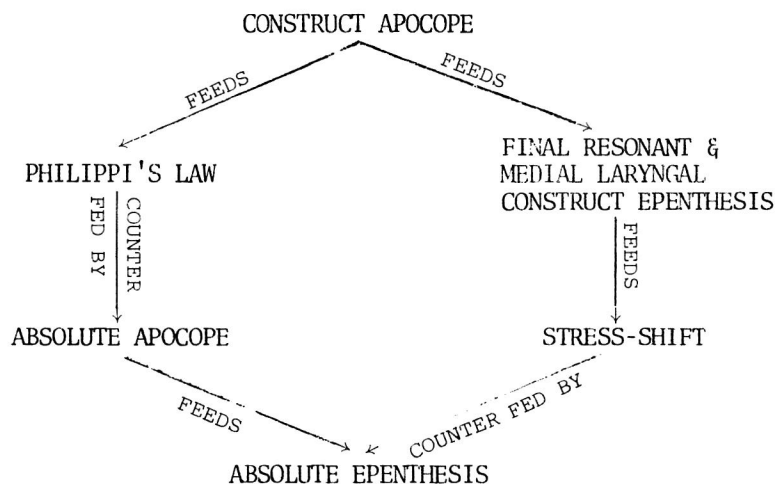


Figure 5

FINAL RESONANT &
MEDIAL LARYNGAL
CONSTRUCT EPENTHESIS

qāḥāθ

-----STRESS-SHIFT

ABSOLUTE EPENTHESIS

(lə)qāḥāθ

-----SPIRANTIZATION

VERB EPENTHESIS

ləqāḥat

Figure 6

Further evidence for Speiser's hypothesis comes from a small group of segolate toponyms, alluded to above (§3.1), which underwent epenthesis early enough to be affected by stress-shift: *šēxēm*, *gāvāl*, and, less certainly, *šāšōm*. That these toponyms are indeed segolates is perfectly clear (except in the case of *šāšōm*) from allomorphs which occur with locative and gentilic suffixes (*šēxmā(h)*, *gāvālī*, and *šāšōmī*²⁸), from transcriptions in the Amarna letters

²⁸This form is listed, without reference, by Yeivin 1973a:145. The source is apparently *Šifra*, Codex Assemani 66, p. 52, where examples appear in lines 8 and 9. The form is spelled with a *šāw* after the *š*, but *šāw* in this manuscript, as in all or most other

(Šakmi, Gubla) and other ancient documents (cf. Koehler, Baumgartner, et. al. 1967, s.v. *gǝvāl*), and, to a lesser degree, from variant spellings in the Isaiah Scroll (*swdm*) and the Genesis Apocryphon (*swdm*, *swdum*) (cf. Kutscher 1959:83-4). Here again we find that all of the forms in question end in a resonant.

It remains to be said that, while it is clear that *šǝxēm*, *gǝvāl*, and possibly *šǝšōm* underwent epenthesis earlier than absolute-state COMMON nouns ending in a resonant, it is not all clear why this should be so. Could it be that proper nouns were used in "pausal" form (i.e. without case-endings) in ancient Hebrew as Brockelmann (1903:5fn1) suggested? Or does the exceptional behavior of these place-names merely indicate that they were borrowed from the dialects of the cities to which they refer (Malone 1971:56, Bauer and Leander [1922]1965:580)? Further research is clearly called for.

A more difficult problem is posed by the existence of COMMON nouns stressed on their epenthetic vowel in the absolute state as well as the construct. It is well-known that nouns ending in *y* generally fall into this category. Since *y* is even more sonorous than the liquids and nasals, it is not surprising that these nouns should be more susceptible to early epenthesis and, consequently, to stress-shift. The only question is whether the oxytone allomorphs spread to the absolute state by analogy²⁹ or whether epenthesis preceded apocope in final-*y* nouns.³⁰

Nouns with medial ² are also regularly stressed on their epenthetic vowel in the absolute state, and much of what has been said about final-*y* nouns applies with slight modification to this class of nouns as well. We might also note that the relationship between the medial-² nouns and the other medial-laryngeal nouns is very similar to the relationship between the final-*y* nouns and nouns ending in a liquid or nasal.

The most difficult problem of all is posed by nouns which are stressed on their epenthetic vowel in the absolute state, but yet do not fall into either of the above classes. Some of these (*gǝvār*,³¹ *šǝlōw*) end in a liquid or semi-vowel, but some (*dǝvāš*, *šǝvōx*, *šǝvāx*?) do not.

manuscripts with Early Babylonian vocalization (Yeivin 1973a:70), often stands for etymological and phonetic ϕ . The vocalization with *o* (although it is possible that in line 9 this has been corrected to *u*) is puzzling (one would normally expect *u* in the Babylonian system), but not entirely unparalleled; cf. Yeivin 1973a:63 (several examples), 202 (*ʔoxle(y)* 'foods'), 204 (*torpə(h)*). Incidentally, the form we are discussing is not a *hapax*. There are many examples of graphemic <*swdmy*> scattered throughout Rabbinic literature (Kutscher 1959:84, Sokoloff [1969]1972:295) which are clearly to be read *suōmī* (Kutscher loc. cit.) or *šoōmī*. Finally, we should note that if we accept Kutscher's derivation (loc. cit.) of *swdmy* from an original **sudumiyyu*, the Sifra's vocalization becomes less difficult to explain, although one might still have expected *suōmīθ* or *šǝōmīθ*. It may, therefore, be necessary, in the end, to abandon our classification of the name *šǝšōm* as a segolate.

²⁹Analogy was more likely with final-*y* nouns than with, e.g., final-*h* nouns, because more final-*y* construct forms had undergone epenthesis.

³⁰In the latter case, we would posit *gadyu* > *gadiu* > *gadiyu*. Changes of this type (which increase the number of syllables in a word) are discussed by Jespersen (1913:193, 198-9). This solution would allow us to account for forms like *šǝvīyyō* (Song 4:5, 7:4) and *gǝšīyyoθāyix* (Song 1:8) without invoking analogy.

³¹This form, which occurs in the phrase *gǝvār tōmīm* (Psalms 18:26) is often said to be a construct form. This view presupposes that *tōmīm* can be a noun as well as an adjective, an assumption which is highly questionable. Not surprisingly the Hexapla vocalizes γαβθ θαμιν.

These forms are anomalous in other ways as well: 1) pre-tonic vowels, even *a*,³² have been reduced (Malone 1971:56fn40), even though the tonic vowels are etymologically short,³³ and 2) stressed *a* has not been replaced by *o* (except before *w*³⁴) even though these are nominal forms. For these forms, the assumption of inter-dialectal borrowing (Malone 1971:56, Bauer and Leander [1922]1965:580) would seem to be the only solution.

4.3. Question 4b (the Vowel *a*)

If we accept Malone's assumption (1971:46) that the original epenthetic vowel was uniformly *e*, we must explain how this vowel was lowered to *a* in *ḥāḏār*, etc. Two possibilities come to mind. On the one hand, we might attribute the lowering of *e* to the presence of *ʾ* (and, of course, the laryngals; cf. Malone's rule (hH), loc. cit.), especially if it took place while the *e* was still unstressed, cf. **wayyāsīr* 'he removed' > *wayyōsar*, and **wayyāʿīr* 'he wakened' > *wayyōʿar*. If, on the other hand, the lowering of *e* took place after the stress had shifted to it, Philippi's law would seem to provide the answer.³⁵

Neither of these solutions is without its difficulties. The former solution is incapable of explaining why the epenthetic vowel of *šāyān* was lowered, while the latter solution fails to explain why the epenthetic vowel of *ḥāvēl* was NOT lowered. Nevertheless, the latter solution seems preferable, since there are other exceptions to Philippi's law which must be accounted for in any case: **ʾāvēl*³⁶ (note the striking phonetic similarity to *ḥāvēl*!), **āqēv*, and *ḥāmēš*. Rather than explain these forms away as analogical restorations (a strategy which, in any event, will not work with *ḥāvēl*, since the corresponding absolute form is not **ḥāvēl* but *ḥēvel*), we propose a solution based on the possibility that Philippi's law and stress-shift partially overlapped in time.

A glance at figure 5 reveals that Philippi's law and stress-shift at least fall within the same general time-period (after construct apocope and before absolute epenthesis³⁷), and that stress-shift could easily be the later of the two. At the same time, it is well-known that stress-shift feeds Philippi's law. We may therefore hypothesize that stress-shift began in time to supply SOME customers to Philippi's law, but that the latter became extinct before the former had worked its way through the entire class of bi-syllabic construct forms.³⁸ In other words, it is possible that we have COUNTER-feeding here as well

³²Short *a*, the most sonorous of the short vowels, is generally immune to pre-tonic reduction in Biblical Hebrew. Final-*y* segolates like *gāḏī* (< **gadyu*) and *šāyī* (< **šabyu*) are only apparent exceptions to this rule, since the original *a* of these forms was raised to *ε* by vowel harmony (cf. *kelyāx*, *telyāx*, *peryāx*, and *gēāī*, *kēīī*, *pēīī*, and the proper nouns *ʿevyāsof*, *ʿevyāsoh*) before being reduced.

³³This last detail, unmentioned by Malone, is crucial, cf. fn. 15, above.

³⁴The *o* in *šāḏōw* may be a product of assimilation (*a* becoming rounded before *w*) at a late stage, rather than a product of stress-lengthening, cf. Blau 1967:63, and add *wayḏōšw* (I Samuel 21:14) to the examples cited there.

³⁵This solution was suggested to me by Norman Didia, a student of mine at Touro College.

³⁶The form which is actually attested is *ʾavel-*, i.e. with *ε* and *maqquf*; cf. p. 7 above for other examples of this alternation.

³⁷I.e., the first stage of absolute epenthesis — affecting nouns ending in a resonant. This must have taken place very soon after absolute apocope. Accordingly, the time-period covered by this diagram is not so long as to invalidate the argument which we wish to base on it.

³⁸Wang 1969, which I came across after writing these lines, argues, on theoretical grounds, that competition between (= the overlapping of) sound-changes is one of the causes of sound-change residue. The Hebrew data presented here certainly seem to support Wang's position.

as feeding³⁹ — and that *hävél*, **ävél*, *äqēv*, and *hāmēš* are "products" of this counter-feeding. As such, *hävél* would no longer stand in the way of a solution to question 4b based on Philippi's law.⁴⁰

5. CONCLUSION

This investigation has shown that forms like *ḥāḏār* and *hävél* are relics of an early stage in the development of Hebrew. The occurrence of these forms in Tiberian Massoretic Hebrew is significant, firstly because some of them (e.g. *šāyār*, *zāḥā*, *nāḏā*, *ḥāsār*) must have already disappeared from colloquial Hebrew by the Mishnaic period, and secondly, because a number of them have been replaced by their absolute-state co-allomorphs in the Babylonian tradition of Biblical Hebrew, e.g. *häväl* (Kahle [1902]1966:171-2), *šāyār* (Yeivin 1973a:193), *šävā* *šrh*, *šävā* *mwt*, *tešā* *mwt* (ibid., 220).⁴¹ These facts must now be added to the growing body of scientific evidence (cf. esp. Kutscher 1959:23-52) indicating that the Tiberian vocalization is a faithful, even slavish, reproduction of a stubborn oral tradition which succeeded in preserving ancient forms even after they had disappeared from the Hebrew spoken by the bearers of that tradition.

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³⁹ This should not be confused with Koutsoudas, Sanders, and Noll's "feeding and counter-feeding" (1974:2), which is a symmetric relation between two NON-OVERLAPPING rules. What we are proposing is that PART of stress-shift precedes the extinction of Philippi's law (and thus feeds it) and part of stress-shift follows it (and thus counter-feeds it).

⁴⁰ Another solution to question 4b might be to reject Malone's assumption that there was only one epenthetic vowel and to assume that, in the earliest period, the epenthetic vowel took on the coloring of the stem vowel (as it does in some Arabic and Akkadian dialects), except when the word ended in a laryngal or *r*. This assumption is made plausible by the fact that *hävél* is apparently a *qītl* form (cf. *hevlō*, *hevlēxo*), while *šāyār* seems to go back to a *qatl* form (cf. Assyrian *šaknu*, from which it is borrowed). But construct epenthesis would still have to be ordered before stress-shift, and stress-shift before the extinction of Philippi's law, and, accordingly, the unlowered *e* of *hävél* would still be a problem. This "solution" is therefore illusory.

⁴¹ Cf. also *šāyār* (abs.) for *šēyār* in *Sifra*, Codex Assemani 66, p. 180. Older forms preserved by the Babylonian tradition are *bāʿā* (Yeivin 1973a:198), *šāḥār* (ibid., 197), and *ḥsār* (Yeivin 1973b:168).

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THE LOSS OF FINAL CONSONANTS IN
BABYLONIAN JEWISH ARAMAIC (BJA) *

by

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The purpose of this paper is to explain the sporadic loss of final continuants in certain BJA forms as owing to metanalysis of forms in which the final consonant was assimilated to the enclitic preposition [l-]. For example, */²marleh/ ('he said to him') developed to */²malleh/, thence to */²maleh/, thence metanalyzed as */²ma/ + /leh/, yielding the new verb form */²ma/ ('he said'). One necessary step in the above development is neutralization of consonant length, implying that such neutralization had already taken place in this dialect. The case for this explanation is strengthened by comparative and intra-dialectal evidence.

One of the striking features of Babylonian Jewish Aramaic (BJA) is that the final consonant of certain verbal roots is often lost. This phenomenon is most common in the late or Geonic dialect.¹ It is more restricted but still well attested in the normal Talmudic dialect, and considerably less prevalent in the special (archaic?) dialect of certain tractates.

All of the consonants so affected are voiced continuants. They are /l/, /m/, /r/, /b/, and /d/.² Examples have been gathered by Margolis and Epstein.³ Among them are

*I wish to thank Professors H.L. Ginsberg, Stephen J. Lieberman, and Joseph Malone for their aid in formulating this paper. Also grateful acknowledgement is due the Maxwell Abbell Foundation for Grants-in-Aid enabling me to pursue my studies. I have also discussed some points with my friends, Prof. Richard Steiner and Prof. Yona Sabar. Of course, responsibility for all opinions expressed is my own.

¹For a general introduction to BJA and catalogue of the sub-dialects see J.N. Epstein, *A Grammar of Babylonian Aramaic*, (Hebrew), ed. E.Z. Melamed, (Jerusalem, 1960) pp. 13ff.

²In Hebrew and Aramaic /b/ and /d/ (as well as /t/, /k/, and /g/) were realized as continuants post-vocally. In BJA the continuant allophone of /b/ was almost definitely a frictionless, voiced bilabial continuant [w] (cf. Malone, *Lešonenu* 37, p. 161).

³M. Margolis, *A Manual of the Aramaic Language of the Babylonian Talmud*, (München, 1910) p. 10. Epstein, pp. 57ff. /n/ and /t/ may be added if we admit the examples in n. 23. In this paper italics will be used for graphemic representation, // for phonemic, and brackets for occasional phonetic. I am assuming only one front vowel phoneme for BJA, an assumption which, while perhaps questionable, does not affect the argument of this paper.

² zl	('he went')	~	² z ²	/ ² za/.
² zyl	('going')	~	² zy	/ ² āzi/.
šql	('he took')	~	šq ²	/šqa/.
šqyl	('taking')	~	šqy	/šāqi/.
² yqbyl	('I will receive')	~	² yqby	/ ² iqabi/.
tyqwm	('it will stand')	~	tyqw	/tiqū/.
q ² ym	('standing')	~	q ² y	/qāj/.
qym	('established')	~	qy	/qi/.
² ymr	('I will say')	~	² ym ²	/ ² ima/.
² mr	('he said')	~	² m ²	/ ² ma/.
nšb	('it blew')	~	nš ²	/nša/.
ny ^c byd	('he will do')	~	nyby	/nibi/.

Many more examples are found in the cited secondary literature, but these will suffice to illustrate the process. To my knowledge, Epstein makes no attempt to account for this phenomenon.⁴

In brief, my explanation is that in certain morpho-syntactic conditions these consonants were assimilated to the initial liquids of common affixes, and later, when long or geminate consonants were not phonemically distinguished from short/nongeminate, the resulting form was re-analyzed and, by analogy, the previous bound form substituted for the previous free form.⁵

Obvious examples of such assimilation are such forms as ²zyn² /²āzina/, ('I am going') or šqyn /šāqina/ ('I am taking') from ²āzilna/ and /šāqilna/ respectively.⁶ It is easy to see why speakers of the dialect would analyze these forms as /²āzi/ and /šāqi/ respectively plus /na/, just as /hāwena/ is /hāwe/ plus /na/.

However, the primary operative morpho-syntactic conditioning for the loss of final root consonants was apparently the preposition [l-] used enclitically. Evidence for the enclisis of the prepositions [l-] and [b-] is found quite clearly in the excellent Hamburg manuscript of B.T. Mas. *Nezikin*.⁷ There we find *hyybbh*, written clearly as one word (3x!), *ptrbh*,⁸

⁴Nor does anyone else of whom I am aware. But see T. Nöldeke, *Mandäische Grammatik*, (Halle, 1875) p. 50 and cf. below n. 20.

⁵Determination of sufficient conditions for this development awaits a full-scale phonological treatment of the language including a distinctive feature analysis. The author is presently engaged in the early stages of such a study.

⁶J.A. Montgomery, *Aramaic Incantation Texts from Nippur*, (Philadelphia, 1913) p. 143.

⁷*The Babylonian Talmud Codex Hambourg* (sic) 165, facs. (Jerusalem, 1969). For an excellent linguistic discussion of this manuscript see E.Y. Kutscher, "Research on Babylonian Aramaic Grammar" (in Hebrew), *Lešonenu*, 26 (1961) pp. 174-177.

⁸Both examples are on *Baba Qamma* 13b. For the orthography cp. Mandaic š²kybbh 'he sleeps in it' with *b* written twice. R. Macuch, *Handbook of Classical and Modern Mandaic* (Berlin, 1965) p. 12.

⁹*ybc'y³ln.*⁹ In the Columbia University manuscript of *Mas. Pesachim*, there is *maqšwlk*, (= 'they object to you').¹⁰ Furthermore, Epstein has recorded a form *pršnlhw*, (= 'we have explained them').¹¹ Also in the important Spanish ms. of *Av. Zar.* published in facsimile by S. Abramson¹² I have found *hwtlly* and *tymrwlyh* both clearly written.¹³ The existence of such enclitic forms ought not surprise us as such pronunciation is clearly indicated in Biblical Aramaic by the Massoretes, through their use of the maqqef, /wye³mar-leh/.¹⁴ An orthography similar to the ones attested above for BJA is already found in the *Gen. Apoc.* 2, 9 *dkrlk*.¹⁵ In Mandaic, enclitic use of these prepositions is the rule, and therefore it is with some degree of certainty that we may say it was in Babylonian Jewish Aramaic as well.

I wish to propose that the final consonant of the verb was assimilated to the /l/ of the enclitic preposition, and that by metanalysis the resulting bi-radical root morpheme came to be used in positions even where free of the enclitic. I have found intra-dialectal evidence for the assimilation hypothesized, in the forms /qillah/¹⁶ and /mašgillih/.¹⁷ It should be pointed out that all verbs would function with the enclitic /l/ in BJA because it marks direct object, and indirect object, as well as being used ingressively in such phrases as /³zal lih/ ('he went').

Therefore the proposed development of these forms would be, for instance (1) /³imar/ ('I will say'), > (2) /*³imarlik/ ('I will say to you'), > (3) /³imalik/¹⁸ > (4) /³ima/ ('I will say'), the most common of these forms. All stages of the process are attested in the above-mentioned form from *Hal. Pes.* where we have also attested *qy ly*.¹⁹ Therefore /qim li/ > /*qimli/ > /qil li/, and finally with simplification of the doubling and metanalysis > /qi li/. Again the transfer of /qi/ and related forms to other environments is easy to imagine and indeed occurs.²⁰

⁹Baba Qamma 20a.

¹⁰*Pesachim* 140b. This excellent manuscript has never been published. See Kutscher p. 177.

¹¹J.N. Epstein, *Gaonische Kommentar zur Ordnung Tohorot*, p. 150.

¹²*Tractate Abodah Zarah of the Babylonian Talmud*, ed. S. Abramson (New York, 1957).

¹³p. 96.

¹⁴Daniel 4, 32. See F. Rosenthal, *A Grammar of Biblical Aramaic* (Wiesbaden, 1968 p. 18 for other examples.

¹⁵N. Avigad and Y. Yadin, *A Genesis Apocryphon* (Jerusalem, 1958) for photograph. n.p.

¹⁶For /qim lah/. /kiwan d³tta la qil lah bšum la samka da³tah/ ('Since a woman is not expert in evaluation she does not depend on it.') *Halakot Pesukot*, facs. (Jerusalem: 1971) p. 106. See Kutscher pp. 173-174. The form *gym l-* in this meaning is of course well attested in Talmudic literature.

¹⁷From *mšgyr lh* ('he sends to him'). L. Ginzberg, *Geonica* (New York: repr. 1968) vol. II, p. 104.

¹⁸Well attested, e.g. B.B. Lewin, *Otzar Hageonim*, (Jerusalem: 1931) Vol. I, p. 61.

¹⁹p. 96. /hani mili hika dla qi li bgawih mar qi li bgawih/ (= That applies when he is not well known to me. Master is well known to me.)

²⁰Although, ironically, not attested with this particular verb.

It must be admitted that it is not completely impossible to imagine a purely phonological solution to this development, i.e. a vocalic realization of these consonants in final position or some other type of simple apocope.²¹ The following considerations, however, render such an explanation considerably less likely. 1) The doubled /l/ in /qillah/ and /mašgillih/ militates against such an assumption. 2) The somewhat skewed distribution of affected phonemes supports the assimilation explanation. While all of the affected consonants could easily assimilate to /l/ (the liquids for obvious reasons, [t] and [d] because of their similar point of articulation to /l/, and /b/ because of its phonetic realization as [w])²², it is hard to imagine why general apocope of voiced continuants would not affect all equally. But see n. 5. 3) There are partially related phenomena in closely related dialects which cannot, by any means, be interpreted in such manner:

"In Modern Mandaic the final *ʔ* of AMR 'to say' regularly assimilates to the encl. (1). Such assimilated forms are then often written phonetically
 "mala (Morg. 273) emalla 'he said to her.'"²³

Mandaic is a dialect intimately related with Babylonian Jewish Aramaic and therefore it seems to me that *ʔmalla* in Mand. cannot be separated from *ʔmalih* ('he said to him') etc. in BJA. Since in Mandaic there is no secondary root AMA, we must interpret this form as assimilation, as of course, the pronounced doubling indicates as well. It follows that the BJA forms are assimilated as well (i.e. and not to be explained as mere scribal survivals of /ʔma/ plus enclitic /lih/, preserved orthographically because of the unusual shortness of the verb form.) Mandaic, then, represents an earlier stage of the process than does BJA.²⁴

In Mandaic, apparently, there was never any back-formation from these assimilated forms, perhaps because of the retention of gemination.²⁵ In fact, the only exact Aramaic parallel I know of to the process hypothesized here is in neo-Aramaic, where in Garbell's formulation, "stem final *m* in the root *q-j/0-m* is replaced by zero (...); further, the objective suffixes of the 2nd p. sg. m. and f. are optionally (but more frequently, except after the negative particle) affixed to the stem: *qu* (→ *qūlox* m. *qūlax* f.), pl. *qūmun* 'rise' (...) the stem of *0-z-l* is supplet by *si-* and takes the suffixes described for *q-j/0-m*; *si* (**ʔsilox* m., *sīlax* f.), pl. *sīmun* 'go'."²⁶ It seems most likely that the suppletion in precisely

²¹Nöldeke, p. 50 does refer to "Abfall" of /m/, and does adduce some seemingly clear cases of it. However, this does not damage our argument appreciably.

²²Cf. n. 2. cp. Neo-Syriac /hal/ 'give!' < /haw/ < /hab+1/. Nöldeke, *Grammatik der Neusyrischen Sprache*, (Leipzig, 1868) p. 256.

²³Macuch, p. 52.

²⁴It does not seem impossible to me that we may parallel this relationship with another. In Mandaic the /t/ of perfect 1st and 3rd feminine is preserved except before the enclitics. (Nöldeke, p. 225). Joseph Malone, in a review of Macuch's book (*Language* Vol. 45, 1969, pp. 197-198) has already demonstrated that *both* forms are to be explained by assimilation. Therefore, it seems likely that the Babylonian Jewish forms, generally without *t*, should be explained as analogical extension of the forms with enclitics. The same may be true for forms like /amrittū/ ('you say'), which again in Mandaic only exist before enclitics, otherwise /amrittun/ is found. (Nöldeke, p. 233). Phrases like /ʔatun ʔamritu/ in BJA point in this direction.

²⁵For rare exceptions see Macuch p. 37. (cp. Malone *op. cit.* p. 200).

²⁶Irene Garbell, *The Jewish Neo-Aramaic Dialect of Persian Azerbaijan*, (The Hague, 1965), p. 71. The /m/ in /sīmun/ is apparently the result of another analogy viz. /qūmun/ analyzed by speakers as /qu/ and /mun/ in spite of the fact that the /m/ is original in the root. (Prof. Y. Sabar, oral communication, 1974).

these two roots and no others is a result of metanalysis of the enclitic forms.²⁷

Finally, it should be noted that there are other assimilation-elision phenomena in Aramaic, such as /nezzun/ < /nezlun/ in Syriac. Such phenomena occur in Babylonian Jewish Aramaic as well in nouns, such as /qola/ < /qolra/, /zuta/ < /zutra/. These are always post-consonantal, pre-vocalic, and are therefore irrelevant here as can be shown from their general occurrence in Aramaic,²⁸ in contrast, of course, to the phenomenon discussed here.

In brief conclusion, the loss of certain root final consonants is to be understood as the result of a syntactically conditioned phonological change, extended beyond its original environments by analogy.

The implications of this analysis, if correct, are two-fold: philological and linguistic. As to the first, Babylonian Jewish Aramaic cannot be used as support for apocope or vocal realization of /r/ in final position. Therefore, for example, it cannot support the interpretation of ʔlʕz (= 'the name' /ʔelʕazar/), in the recently published Gibʕat Mibtar inscription as /ʔelʕaza/.²⁹

As to the second, we would have to conclude that gemination must already have been non-phonemic in this dialect in order to allow such metanalysis.³⁰

²⁷The phenomenon would perhaps be more widespread in the dialect were it not for the fact that the /rl/ combination became /rr/ > /r/ in this dialect (Garbell, p. 70) therefore /mirox/ 'you said' not /*milox/. The metanalysis was therefore prevented.

²⁸C. Brockelmann, *Grundriss der Vergleichenden Grammatik der Semitischen Sprache*, (Berlin, 1908) Ip. 159.

²⁹E.S. Rosenthal, "The Inscription from Gibʕat Mibtar," *Peraqim* 2, (Jerusalem: 1975), p. 343, n. 31. I do not wish, of course, to deny the correctness of the interpretation, only to question the support from BJA.

³⁰Cp. Syriac /ntl/, certainly from *ntn+l* > *ntl+l*, where when false division takes place the /l/ goes with the root because of the gemination.

THE POSITION OF THE DECLINED *kl* IN SYRIAC

by

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The position of some types of noun modifiers in Aramaic and Syriac does not always correspond to strict rules. The main types of noun modifiers of this kind are: the demonstrative pronouns, the numerals and *kl*. We shall try to prove that so far as concerns the third type, *kl*, some rules can be established.

Nöldeke writes that "Überaus oft hat ein Substantiv *kl* mit seinen Pronominalsuffix in Apposition neben sich, voroder nachgesetzt."¹ An examination of the Peshitta to the Pentateuch shows that *kl* always precedes the nucleus, except for the following cases:

(a) When the nucleus is a genitive construction, in which the second member is also a genitive construction or a relative clause. Examples: Lev. 4:8 *wtrb² klh dtwr³ dhth⁴* 'all the fat of the bullock' (but at the end of the same verse *wklh trb⁵ d⁶l gwy⁷* 'and all the fat inwards'); Gen. 3:2 *dmn p⁸ry yln⁹ dbprdy¹⁰ klhwn n¹¹kwl* 'we may eat of all the fruits of the trees of that are in the garden' (but Gen. 2:16 *klhwn yln¹² dbprdy¹³* 'all the trees that are in the garden'). As was already mentioned, when the nucleus is a genitive construction composed of only two members, it will be preceded by *kl*. An exception is Gen. 41:36 *dnhwyn b¹⁴r¹⁵ klh dmsryn* 'which will be in all the land of Egypt.' This is not a translation of the Hebrew, since *kl* is not found in this verse. But *kl r¹⁶s mšrym* is always given as *klh r¹⁷ dmsryn* (e.g. *ibid. ibid.*:29, 54, 55). It may be suggested that where the translator had not before him the Hebrew *kl*, he felt himself free to deviate from the Hebrew order of words (see, for example, Num. 20:11). It seems therefore that the position of *kl* is influenced by Hebrew, at least as far as the examples where the nucleus is composed of one word only are concerned.

(b) When the nucleus is composed of a substantive and the demonstrative pronoun (e.g. *hn¹⁸*).

Four constructions are here possible, of the type: (1) *hn¹⁹ klh ywm²⁰*; (2) *ywm²¹ hn²² klh*; (3) *klh ywm²³ hn²⁴*; (4) *klh hn²⁵ ywm²⁶*. Constructions of the type *ywm²⁷ klh hn²⁸* or *hn²⁹ ywm³⁰ klh* were not found.

Some examples: (1) Gen. 31:1 *qn³¹ hn³² klh qnyn³³* 'he got all this wealth (according to the Ambrosian codex²; Lee³, as well as the Urmi and Mosul editions, read *hn³⁴ qnyn³⁵ klh*); Lev. 18:26 *hlyn klhwn hth³⁶* 'all these sins'; Num. 5:30 *hn³⁷ klh nmws³⁸* 'all this law'. (2) Gen. 33:8 *yymk³⁹ lk mšryt⁴⁰ hd⁴¹ klh* 'from where do you have all this camp' (again the Ambrosian codex has been checked. Lee, Urmi and Mosul read *hd⁴² klh mšryt⁴³*); Ex. 11:8 *m⁴⁴ hn⁴⁵ klh* 'all these people'. (3) Ex. 10:13 *klh ymm⁴⁶ hw* 'all that day'; Ex. 11:8 *klhwn bdyk hlyn* 'all these your servants'.

¹T. Nöldeke, *Kurzgefasste syrische Grammatik*, 2d. ed. (Darmstadt, 1969), §218.

²*Translatio syra Pescitto ... ex codice Ambrosiano* (Milan, 1476).

³S. Lee and G.E. Barnes, *Pentateuchus syriace* (London, 1914).

(in group (2) above we find a different construction, which might be explained as a variety);⁴ Ex. 19:7 *klhwn ptgm² hlyn* 'all these words' (compare also Ex. 24:8). (4) Examples of this type are to be found only in Deuteronomy: 3:21 *lkwlhyn hlyn mlkwt²* 'to all these kingdoms' (Lee, Urmi and Mosul read *lhlyn klhyn mlkwt²*); 6:25 (and passim) *klh hn² pwqdn²* 'all this commandment' (Lee, Urmi and Mosul have *pwqdn² hn² klh*).

Examples of the group (2) are quite rare; therefore we may still maintain that *kl* precedes the nucleus.

(c) When the nucleus is the demonstrative pronoun.

It seems that under the influence of the *hn² klh ywm²* type, *kl* may follow the demonstrative pronoun even when it is the nucleus: Lev. 10:19 *wmtyny hlyn klhyn* 'and all these have befallen me'; Lev. 18:24, 24 *hlyn klhyn* 'all these'; Gen. 41:39 *hd² klh* 'all this' (but here, too, usually *kl* precedes, e.g. Gen. 10:29, 25:4).

(d) Except for what was mentioned in the above three groups only very few exceptions can be found where *kl* follows: Lev. 8:21 *w²sqh mwš² ldkr² klh* 'and Moses burnt the whole ram'; Num. 14:21 *w²tmly šwbhnh dmy² b²κ² kwlh* 'and all the earth shall be filled with the glory of the Lord.'⁵

⁴Cf. the writer's article, "Problèmes de variation dans la traduction de la Peschitta sur le Pentateuque," *Semitica* 25 (1975).

⁵These notes may be considered as a contribution to the general problem of noun modifiers. Takamitsu Muraoka, "Remarks on the Syntax of some types of noun modifiers in Syriac" (*JNES* 1972,3), treats the problem of the numerals (and also adjectives + demonstrative pronoun) modifying substantives. See also my article "The position of the demonstrative pronoun in Syriac" (*JNES* 1975,2).

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